Answers to reviewers:

Reviewer 1 :

The manuscript describes the model development of a coupled soil-atmosphere model based on Ross (2003) solution for the Richards equation and the De Vries (1963) soil energy balance equation. The model should be used for as predictive software, and therefore, computational time should be reduced. After model description, the newly developed model was compared to an existing model (TEC of Chanzy and Bruckler, 1993). In general, the topic and the research presented are appropriate for publication in HESS. Unfortunately, the manuscript does not always match all scientific criteria for publishing (see specific comments). Therefore, the I recommend major revisions.

General Comments:

The general, the introduction should be carefully revised (see specific comments).

It seems quite evident that we failed to express clearly the objectives of the paper. It order to remediate this issue, the introduction was completely rewritten to:

- Express clearly and directly the objective of the paper which is to evaluate if switching the regular Richards equation to a generic (therefore extended) Ross solution within a coupled model did not lead to significant discrepancies in results.
- Limit the literature reviews on side subjects which may create confusion, and therefore be more coherent altogether. Specifically, discussion on PTF was partly moved to the model description as well as working hypotheses.

The major drawbacks I do see is the fact that the model mainly focus on faster computation of the Richards equation and the coupling to a heat transport equation while other processes necessary for precise prediction of the soil water balance and state are neglected such as plant growth and root water uptake. Additionally, I do not see the necessity to increase model complexity by including the heat balance equation while neglecting important processes such as root water uptake. Additionally, sources of uncertainty are also mentioned in the introduction such as uncertainties associated by the choice of PTF while other sources of uncertainty (and maybe more important ones) such as unknown rooting depths, root water uptake, atmospheric forcings are not discussed.

We understand the importance of vegetation related processes (root water uptake, transpiration, crop development…) when dealing with agronomical modeling. Such consideration of the vegetation leads to strong uncertainties related to the plants “geometry” and the plant behavior in regards to water content and atmospheric forcing. Moreover, and unlike the case of soil mass balance or soil energy balance, there is no consensus on how root distribution, root water uptake and transpiration should be modelled. Therefore, we believe that the study of such processes requires a focus on their own. This is the reason why we chose to first validate our model on bare soil, studying specifically other processes. In particular, we studied the coupling with heat transfer, which is necessary to allow a coupling with surface energy balance and thus have the capacity of driving simulation through atmospheric forcing. That said the developed model, as it names suggest, is further developed to account for vegetation. Details on the choice of bare soil were added to the introduction and mention of the development of vegetation related models in present in the perspectives.

Actually, I do not get the full message of the ms. There is a strong focus on the coupling between water and heat transfer but only the water flow will be evaluated later on. This problematic are also stated in P8579 L 19 to 25. Therefore, the data and the scope of the ms do not really fit together. Finally, I would suggest benchmarking the new model (or implemented Ross approach) versu analytical solutions for different boundary conditions instead of benchmarking versus a model which might be set up in a different way such as grid discretization etc. (see also special comments later). As far as I understand the major advantage of the new model approach is fast computation.

The Ross solution has already been benchmarked against analytical solution (Varado et al., 2006a) as well as the soil energy balance module. However, to the authors' knowledge there is no analytical solution for a coupled model such as the one studied here. Initial evaluation of the model included both validation against analytical results (in non-coupled conditions) and against experimental data (similarly to the work done with TEC). However, the objective of the study is to see if the degradation of the resolution method for Richards' equation using Ross solution and looser coupling allowed acceptable results. To this end, we need a benchmark considering heat and mass transfers and a way to consider different soil hydraulic characteristics. This is not possible with analytical solution and comparison with experimental data leads to questioning the parameterization (of modeler's choices) rather than the model itself. Therefore, the flexibility and the physics of TEC are appropriate to do such a comparison.

We tried to be more explicit on the choice of the benchmarking within the introduction of the TEC model.

Therefore, I wonder why only mass balance errors are shown as indicator for the goodness of the model instead of comparing CPU time of the models for same setups and problems.

More information on the efficiency of the Ross solution in the coupled model was added. However, for detailed work on the efficiency of the Ross solution, we refer to the works of Ross (2003) and Crevoisier et al. (2009) who demonstrate the efficiency of the Ross solution and its ability to deal with coarse grids. Moreover, since we used already published results on TEC, the computation was not done on the same hardware (neither with the same configuration of the hardware). Both computers were 'regular' desktop computers but all these points influence the results on computation times. Therefore, due to all this uncertainties, we choose not to present detailed results on computation time but orders of magnitudes.

Finally, I would like to point out that proving the efficiency of Ross solution was not the objective of the paper, but rather that using an efficient (as demonstrated in literature) model such as Ross in a coupled and more generic model would not lead to significant errors in numerical results.

In general, either use soil moisture or soil water content. Please be consistent within the manuscript.

Modifications were made accordingly.

Specific Comments:

P1 L17: what was the outcome of the day detection?

The following points were added to the text as suggested:

"

The ability of the models to detect the occurrence of soil water content thresholds with a one day tolerance was also evaluated. Both models agreed in more than 90% of the cases."

In general, the introduction needs revision at some parts. I also miss out a critical review about processes which should be accounted for to reduce uncertainty and processes which are not accounted for.

Precisions regarding processes and a more precise description of the scope were added to the introduction (see previous comments).

P8572 L21; add comma before whereas.

Done

P8572 L24. Should be: actual water content.

Done

P8572 L25: : : :soil moisture probe development: : : any references for this?

This reference was added:
Evett, S.R. and Parkin, G. (2005) Advances is soil water content sensing: the continuing maturation of technology and theory. Vadose Zone Journal, 4, 986-991

P8572 L26: : : :spatial soil variability: : :. any references for this?

This reference was added:
Evett, S.R., Schwartz, R.C., Tolk, J.A. and Howell, T.A. (2009) Soil profile water content determination: Spatiotemporal variability and neutron probe sensors in access tubes. Vadose Zone Journal, 8(4), 926-941

P8573 L1: I agree that modelling SWC is essential but for predictive purposes spatial soil heterogeneity should be accounted for using either full 3D or distributed 1D models. Please discuss carefully.

We agree that 3D / distributed 1D models are necessary for a more precise description of the problem. The question of the efficiency of the model is even more important since the computation time increase significantly with the amount of 1D models or with the dimensions. Both models may be used in 3D, however we limited ourselves to a 1D study. Indeed, the aim of the paper is to evaluate the accuracy of FHAVeT against TEC rather than establish the effect of parameterization or modeler's choice. Comment on extension to 3D / distributed 1D models and the advantages of FHAVeT for such uses is added to the introduction.

P8573 L9: this might be only true if you do have a bare soil. If you would have crops a scientific sounding module for crop growth and root water uptake is also mandatory.

See previous comments (General section) on the modeling of vegetation and the aim of this work.

P8573 l18: especially upward flow is often neglected in capacity models leading to less water available in the root zone during dry conditions, especially if the water table is shallow.

A comment was added in this regard in the document.

P8573 L23: should be: : : :water retention and the hydraulic: : :.
Done

P8573 l24: should be: : : :of these parameters and the hydraulic: : :.
Done

P8573 L25: Rosetta (by Schaap et al) might be one of the mostly used PTF.

This reference was added:
Schaap, M., Leij, F.J. and van Genuchten, M.T. (2001) ROSETTA: A computer program for estimating soil hydraulic parameters with hierarchical pedotransfer functions. Journal of Hydrology, 251, 163-176

P8574 L1: : : :under discussion: : : especially for wet conditions where preferential flow occurs.

Done

P8574 L22: reference for the SiSPAT model missing

This reference was added:
Braud, I., Dantas-Antonino, A.C., Vauclin, M., Thony, J.L. and Ruelle, P. (1995) A Simple Soil Plant Atmosphere Transfer model (SiSPAT). Journal of Hydrology, 166, 213-250.

P8574 L23: same for the Hydrus model

This reference was added:
Simunek, J., Sejna, M., Saito, H., Sakai, M. and van Genuchten, M. Th. (2008) The HYDRUS-1D software package for simulating the one-dimensional movement of water, heat, and multiple solutes in variably-saturated media. Department of Environmental Sciences. University of California Riverside. Riverside, California.

P8574 L24-26: This sentence is somehow out of line. Please rephrase in put it into the context. Does it play a role if BC will be used?

The sentence was moved to the discussion on PTF. See later comments on the role of PTF.

P8574 L27: Ross resolution. Do you mean solution?

Done.

P8575 L1-2: It is clear that different hydraulic properties will influence the outcome. For me it would be more important to know why the different PTF behave so differently.

In the original Ross description (and this is noted as a drawback of their method in the 2003 paper), the only Brooks and Corey type hydraulic curves may be used. In the Ross solution, the Kirchhoff potential (which requires integration of the hydraulic conductivity against soil potential) is necessary. When using Brooks and Corey description for the hydraulic characteristics, this integration is straightforward and can be done analytical. This is not true when using Van Genuchten – Mualem curves. However, many pedotransfer functions are developed for Van Genuchten – Mualem description. Therefore, in order to have a generic model that may be used with several PTF (such as the ones of Wosten et al.(2001) or ROSETTA (Schaap et al. 2001), an extension of the Ross description, and mainly the numerical calculation of Kirchhoff potential is necessary. Crevoisier et al. (2009) already added the use of Van Genuchten – Mualem with the restriction of $\eta = 0.5$ and using a numerical integration method. For this work, we allowed an even more general use of PTF since we developed a numerical method for Van Genuchten – Mualem with $\eta \leq -1$ (Schaap et al. (2001) noted that fits generally lead to $\eta \sim -1$) and a more exact solution to Van Genuchten – Mualem fonctions for $\eta > -1$ using beta functions.

The aim of our work is not so much to compare different PTF (such work has been done previously, for instance in Chanzy et al. (2008)) but to evaluate if FHAVeT allows most generic PTF to be used with little discrepancies compared to TEC.

In regards to variation due to the use of different PTF, we would like to refer to Chanzy et al. (2008) who provide a literature review on such issues. It should be noted that PTF were built against different databases from different regions in the world. Moreover, the PTF are not necessarily fitted in the whole range of saturation for both water retention and hydraulic conductivity which may lead to extrapolation, which can be very different depending on the formulation of the hydraulic characteristics.

The manuscript introduction was revised to clearly define the objective of the paper. The added capacity of the model in regards to PTF was developed in the model presentation (subsection 3.3).

P8575 L6-7: here the question arises whether there is more uncertainty propagated through the model by using different PTFs or variability in the atmospheric forcings. It is widely known that atmospheric forcings (especially precipitation) are highly variable in space.

We agree with the remark. However in operational implementation a lot of assumptions must be done to run soil water transfer models. The determination of soil properties is a major difficulty and we wish to upgrade the Ross model to be adapted to a range of soil hydraulic functions to take profit of existing PTFs and leave a choice to the users. In any models there are a number of sources of uncertainties. Aside from the uncertainties due to atmospheric forcing, uncertainties may be due to parameterization, vegetation, time evolution of characteristics (porosity for example may evolve with rooting), and spatial variation of characteristics as well as spatial water redistribution through runoff. Dealing with those uncertainties are a critical point. However, it would require a specific analysis and dedicated reference data to fully consider all parameters and their evolution with space and time directly from experimentation and / or direct measurement. This would be the body of another study with a specific design. Data assimilation is likely a way to overcome such sources of uncertainties and efficient models allow a more practical implementation of data assimilation.

The added value of the development of an efficient coupled model such as FHAVeT for other techniques (like data assimilation) is described in the introduction.

P8575 L7-10: This sentence is out of line.

See previous comments on the role of PTF.

P8575 L12-13: I do agree that a full representation of all physically based processes would lead to the most exact solution but does it make sense for predictive software?
Maybe other processes such as crop growth and root water uptake are much more important compared to the head balance. Please discuss carefully.

A discussion on semi-empirical models drawback is made in the document and especially highlights that some parameters are not measurable. Non-physically based models may become predictive if fitted against data but will lose this capacity if there is a significant change (nature of crops, climatic change, site geometry change …). Therefore using physically-based models is essential to allow predictability and versatility of the model. Representing all of the processes may not be always necessary. But it allows a more general description and once again more versatile software. Clearly, when working on agricultural management a crop development and root water uptake module are necessary to the model. Such modules exist; however, for the paper presented we chose to focus on mass and heat balance and therefore limited ourselves to bare soil. Discussion on the choice of the model and processes is made in the general comments.

We revised the introduction altogether to point out more clearly the objective of the paper and the working hypotheses.

P8576 L1-6-8: that's not a hypothesis. You stated the hypothesis later. Please rephrase.

Done

P8577 L2: Shouldn't it be Eq.1?

Done

P8577 L12: air or soil temperature? Please be precise.

Precision was made with the subscripts.

P8577 L22: Shouldn't it be Eq. 3 to 5

Done

P8578 L7: Should be Darcian flux

Done

P8578 L18: Should be actual heat transport parameters.

Done

P8578 L21: How do you treat the air phase?

For the soil mass balance air phase is considered at equilibrium as it is always considered when using Richards equation (it is actually an hypothesis used when developing the equation). For the soil energy balance, the air phase is neglected for both the capacity (there is about 3 order of magnitude difference in heat capacity weighed by density of air compared to other phase due to contrast in density) and convection terms. This assumption is very classic when modeling heat transfer in porous media. Moreover, as stated in the paper, heat conductivity is dependent of soil water content and therefore the evaluation of the conductivity term takes into account the air phase. That said, we noted an approximation in our detailing of the equation, and therefore we added the more accurate description as follows:

$$\left(\rho C_p\right)_{eq} = \rho_h C_{eq} = \theta \rho_w C_w + (1 - \theta_s)\rho_s C_s$$

Where ρ_s and C_s the solid density and heat capacity respectively and ρ_h is the soil bulk density.

P8579 L4: I am aware that most models assume rainfall having either a constant or air temperature (both assumptions are wrong). Can you later comment on this limitations?

We consider a constant rain temperature to be a working hypothesis. It should be noted that the model could handle an evolutive rain temperature but we did not have any data to support such a characterization. An inexact value for rain temperature may lead to inaccurate description of the surface heat fluxes especially during heavy rain periods or if a water layer is formed over the soil. However, we would like to point out that, in his thesis, Mumen (2006) showed that thermal boundary conditions had relatively little effect on water content.

Ref: Mumen M. (2006), Caractérisation du fonctionnement hydrique des sols à l'aide d'un modèle mécaniste de transferts d'eau et de chaleur mis en œuvre en fonction des informations disponibles sur le sol. PhD Thesis, 169 pp., Université d'Avignon et des Pays du Vaucluse, Avignon, France.

The constant temperature was clearly identified as a working hypothesis within the document.

End of paragraph: In general, it is not clear to me how the two models differ. It seems that FHAVeT is a 3D model (later used in a 1D mode) but how is it with TEC. Maybe some more words are necessary to introduce both models and to clarify differences.

Technically, both models could be used in 3D. The main differences between the two models may be summarized as follows:

- The soil energy and mass balances coupling is different. In TEC we used a de Vries approach which leads to the computation of water content (and soil potential), soil temperature and vapour pressure. FHAVeT uses a loose coupling neglecting the vapour transport. The consideration (or not) of the vapour flux is one of the major difference.
- The equation method of resolution is different. In TEC, a Galerkin Finite Element Method with an implicit scheme is used, whereas in FHAVeT the Ross method is used (for the mass balance).

In the introduction, we highlighted the added value of our model compared to previously developed model (including TEC) as well as a justification of the choice of TEC as a benchmark. Also in the description of TEC model we developed the differences between the two models.

P8580 L8: should be: ranging

Done

P8580 L19 and 21: should be normal n for the shape parameter (same also in Table 1).

The term is the Greek letter η and is different from parameter n. This parameter is occasionally referred as “tortuosity” and sometimes labelled L. It is often documented with a value of 0.5, but Schaap et al. 2001, for instance stated that generally fits point towards a value of -1.

P8581 end of upper paragraph: How did you treat the lower boundary? Did you test on grid convergence?

The setup (discretization, boundary conditions, initial conditions, …) used for both models are developed in the “model intercomparison” section. This section was subdivided into subsections for easier reading. One of this subsection is labeled “model setup” and contains all necessary informations. The choice behind the boundary conditions was well developed and justified in the work of Chanzy et al. (2008). Considering the aim of the paper no further development was added to the text. A study of discretization effect for Ross solution was done in the paper of Crevoisier et al. (2009). Compared to this study, we chose to use a fine discretization in order to limit the numerical shortcomings of a poor discretization scheme (whether on soil mass or soil energy balance). However, it should be noted that the work of Crevoisier et al. (2009) demonstrate that Ross solution allows a coarse discretization scheme. In the case of the TEC model, the discretization scheme is also fine and moreover (as described in the paper) is refined close to the surface to limit numerical errors or divergence due to discretization.

P8581 L16-17: I do not understand. Later you stated that the accepted error threshold should be 1%. So why does the TEC does not hold this threshold (I do see only mass balance errors <1% for the TEC model).

Figure 3: The mass balance cannot be expressed in %, indeed its unit is in m^3/m^2 due to the 1D description, modification was however made in the text for coherence. Moreover, the figure scale was extended to show the points with errors > 0.01.

Also the question arises whether the mass balance error in the TEC model is a consequence of solving the Richards equation numerically (so called solver problems) or if the mass balance errors are a consequence of the grid discretization (too large grid sizes close to the surface).

Therefore, I would suggest not to benchmark the FHAVeT model versus any other model (here the TEC model and use the mass balance) rather than using analytical solutions which do exists not only for water but also for heat flow for various boundary conditions (e.g. benchmark over BC flow) in a first step. Additionally, the points shown in Fig. 3 are only selected mass balances for predefined time steps. As far as I understand mass balance was calculated as the absolute error. To my understanding large positive and negative errors can also compensate each other and might lead to an overall small error. If the time step for calculating the mass balance is large the overall balance might be still OK but the timing of the water flow might be wrong. Is this right?

See answer on bencmarking in the general comments.

In regards to the mass balances in TEC both sources of errors (solver problems and grid discretization) must be considered.

The mass balance is calculated as follows:

$$\varepsilon = \max_t \left| (V_{wat}^{t} - V_{wat}^{0}) - \int_0^t (Q_{prec}^{\tau} - Q_{evap}^{\tau} - Q_{drainage}^{\tau}) d\tau \right|$$

In other words, the mass balance error corresponds to the maximal value of the difference between variation in soil water content from the initial time and accumulated boundary fluxes. Considering we chose the maximal value rather than, for instance, the final value we tend to consider the worst conditions. Clearly, the error may be compensated along time, but when the error occurs (for instance if the timing of infiltration during precipitation is off) it would show and the mass balance error would be affected. Moreover, other metrics later shown in the paper tend to demonstrate a rather accurate timing.

P8581 L22: Should be Richards equation

Done

P8581 L26: should be conductivity curve

Done

P8582 L8: in Figure 4 the WC after 2 hr are shown. Why did you select this time? Why not show all data for all times?

Figure 4: The results are used every two hours in order to allow significant description of the accuracy of the models with time variation. The legend of the figure was modified to point out the results were outputted every two hours.

P8583 L4: I agree that vapour transport might play an important role leading to the differences observed. But again can you exclude any other influencing factors leading to differences in flux or state such as differences in grid settings or time step control (actually affecting the mass balance)?

We agree that the lack of vapour transport may not be the sole possible reason for the discrepancy. However, the fact that such an error does not appear systematically and only in drying conditions tend to point towards that direction. Other possible causes were added to the paper with a justification as to why we consider the lack of vapour transport as our main suspect.

P8583 L5: should be: : : :,that the model: : :

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P8583 L10: should be: : : :for each model under drying consitions.

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P8583 L11: maybe better: : : :are comparable below 30: : :..

Done

P8583 L15: I do not fully agree. For sure the profiles will correspond much better after infiltration but how is it concerning fluxes over the BC (upper and lower) for the entire simulation period. And how does these differences in fluxes over the BC affect root water availability?

We agree that our interpretation of the results might have been too optimistic. We modified the analysis to a more prudent and reserved approach. Specifically, we eliminated the subjective comments such as “This may demonstrate that the neglected volume of evaporated water is not very important in regards to the total amount of water”. We however added some objective metrics. Namely, the maximal water content discrepancy is of 0.087 m^3/m^3 in the dry state, compared to 0.015 m^3/m^3 in the wet state (in other words the local maximal error is about six times higher in the dry state than in the wet state). The total water volume (on the whole domain) difference between the two models is of 0.0071 m^3/m^2 in the dry state and 0.0052 m^3/m^2 meaning that 27% of the missing water was recovered in a matter of 8 days. This tends to show that some of the water is recovered though not all, moreover the error in terms of water content is diluted along the domain.

The effect on evaporation fluxes may be observed in Figure 7. This figure shows that the evaporation fluxes are off during the drying period but do not demonstrate stronger discrepancy after the drying period (in comparison to before the drying period). Finally, further impacts on this subject are evaluated to the decision making. Specifically, the decision making evaluation demonstrated that while decision may be off during the drying period this is not the case after reinfiltration.

All these considerations were added to the text.

P8583 L16-17: maybe better: : : :for agronomic management such as irrigation.

Done

P8583 L18: maybe better: : : :status reaches: : :.

Done

P8584 L2: maybe better: : : :threshold is reaches

Done

Tables:
Table 4: is the wind velocity the mean velocity? Please indicate.

Modifications in table 4 was made accordingly

Figures:
Figure 3: why not express the mass balance error directly in %. Would be more intuitive.

The mass balance cannot be expressed in %, indeed its unit is in m^3/m^2 due to the 1D description, modification was however made in the text for coherence. Moreover, the figure scale was extended to show the points with errors > 0.01.

Figure 4. Why use the WC after 2 hr?

The results are used every two hours in order to allow significant description of the accuracy of the models with time variation. The legend of the figure was modified to point out the results were outputted every two hours.

Reviewer 2:

1 General comments

The paper describes a newly developed model coupling designed to accurately simulate soil moisture and energy balance to aid in the prediction of timings for agricultural management while begin computationally efficient. The material is relevant to current research specifically in agricultural research but also to other studies which consider soil atmosphere exchanges. However, I have significant concerns over the stated aims and the validation / evaluation. My concerns broadly cover two areas: 1) the lack of comparison with empirical data 2) an apparent disconnect between the stated objectives and parts of the results and discussion sections, and 3) the text gives the impression that the authors are not clear which objectives are the most important to the study.

The paper should only accepted for publication after the below concerns have been addressed.

2 Specific comments

Overall the manuscript needs to be more concise, in particular there are elements of repetitious which could be removed. Moreover, a more concise text will have additional space to address the comments below.

It seems quite evident that we failed to express clearly the objectives of the paper. It order to remediate this issue, the introduction was completely rewritten to:

- Express clearly and directly the objective of the paper which is to evaluate if switching the regular Richards equation to a generic (therefore extended) Ross solution within a coupled model did not lead to significant discrepancies in results.
- Limit the literature reviews on side subjects which may create confusion, and therefore be more coherent altogether. Specifically, discussion on PTF was partly moved to the model description as well as working hypotheses.
- To focus the analysis on the paper objectives

The structure of the paper was revised to limit repetitive elements (for instance on the mass balance description and the use of PTF). The Ross model was also described in a simpler way, referring to the work of Ross (2003) and Crevoisier et al. (2009) for details.

The FHAVeT model is repeatedly (including the title) referred to as a "…couple model for soil atmosphere…" or "..coupled soil atmosphere model…" which implies there are feedbacks between the atmosphere and soil processes. This is misleading as the atmosphere acts only to provide forcing to the model. While I accept that there is coupling between different model components of soil hydrology and energy balance, the fact that the atmosphere is not really coupled to the model should be made clear.

We agree and update the paper accordingly.

The abstract does a good job of justifying the need for robust means of simulating soil moisture however I remain unclear exactly what the model described in the paper offers over existing systems

Our model has too major developments compared to existing models:

- Coupling the Ross solution with surface energy balance (and soil energy balance). This coupling is common with the regular solution for Richards equation but new with the Ross solution.
- Extension of the soil characteristic curves to Van Genuchten – Mualem (with $\eta \neq 0.5$), that allow the use of classic PTF such as the one developed by Wosten et al. (2001) or ROSETTA. Considering soil characteristics is not straightforward when using Ross solution as it requires the calculation of Kirchhoff potential (integral of the hydraulic conductivity over soil potential). The Kirchhoff potential can be calculated analytically when using Brooks and Corey description but requires numerical methods for the Van Genuchten – Mualem case. Ross (2003) points out this shortcoming of his model.

Moveover, there are no empirical results within the abstract to justify the claims that the model is useful in achieving the stated objectives. The abstract would be greatly improved if some reference to the results are made, such as the compute time, errors between soil states or predicted management timings and observational data etc are made.

Metrics (on mass balance and day detection success rate) were added to the abstract.

The description of the TEC model is important and relevant given the TEC model is being used for evaluation and that there is no data currently presented. Given that the TEC is itself a model and is not perfect it would be good to include comparison with data from the simulation agricultural sites (i.e. soil temperature, moisture content, evaporative fluxes?), to justify the results shown here.
Moreover, given the lack of any observational data the description of the TEC reference model needs to be extended (page 8579, lines 5-17) There needs to be some justification of why the TEC model is appropriate to use as a substitute for actual data.
While there is a reference pointing to a comparison between observational data and the TEC model, page 8579 line 22-24, there is no indication of how well the TEC model performs. This is important given differences between the FHAVeT and TEC models are being attributed to 'errors' in the FHAVeT e.g. page 8583 line 5-7.

As pointed out by the reviewer, the model TEC has itself been validated against experimental results (in Chanzy et al. 2008), which is the reason why we chose TEC as benchmark. Initial evaluation of the model included both validation against analytical results (in non-coupled conditions) and against experimental data (similarly to the work done with TEC). However, the objective of the study is to see if the degradation of the resolution method for Richards' equation using Ross solution and looser coupling allowed acceptable results. To this end, we need a benchmark considering heat and mass transfers and a way to consider different soil hydraulic characteristics. This is not possible with analytical solution and comparison with experimental data leads to questioning the parameterization (of modeler's choices) rather than the model itself. Therefore, the flexibility and the physics of TEC are appropriate to do such a comparison.

We tried to be more explicit on the choice of the benchmarking within the introduction of the TEC model.

Page 8574, lines 16 to page 8575, line 20 deal with the Ross (2003) proposed method for solving the Richards equations and subsequent developments of the approach.
However, given that the ultimate decision of the authors is to use an approach based on the original Ross model (page 8587, line 21-22), the level of detail given seems excessive. Please attempt to be more concise.

In the introduction we tried to limit the description of side considerations (such as PTF and coupling) that were partly moved to the model description section. Moreover the equation description of the model was removed and a more detailed description of the extensions that were done to the original model was added. For a detailed description of Ross method, reference to Ross (2003) and Crevoisier et al. (2009) is made.

Page 8576, lines 6-11, explicitly state that the different model functions will be evaluated by considering soil moisture accuracy and timing in decision making based on soil moisture. Figure 9 shows results of differences between the day of a threshold event being simulated between the two models, however I am unable to find any detail of what management events are actually being simulated, i.e. ploughing, sowing, harvest? Based on the simulation dates, I assume that sowing date is the intended target. However further detail is needed, including some information on the uncertainty associated with the target values. Given the explicit statement that the paper aims to develop a model which effectively predicts timings for management practices additional detail is on your evaluation criterion is required. Particularly given that I find it difficult to believe that soil moisture would be correctly simulated given the lack of vegetation in the model.

You noted that we were not clear as to which applications we are aiming for in regards to the decision making. Considering we chose to focus on bare soil (justifications are detailed in the general comments), the target would indeed be tilling and sowing. As future work for the model, description of vegetation is being done, which would to more targets to aim for.

The end of the discussion introduces the need for subsequent inclusion of vegetation to deal with "...water transfer due to vegetation." Given that the presence of vegetation should also impact significantly on surface energy balance through impacts on albedo as well as turbulent exchange, have you simulated any periods where vegetation is present on the ground? If so, do these periods coincide with periods of increased error?

We understand the importance of vegetation related processes (root water uptake, transpiration, crop development…) when dealing with agronomical modeling. Such consideration of the vegetation leads to strong uncertainties related to the plants "geometry" and the plant behavior in regards to water content and atmospheric forcing. Moreover, and unlike the case of soil mass balance or soil energy balance, there is no consensus on how root distribution, root water uptake and transpiration should be modelled. Therefore, we believe that the study of such processes requires a focus on their own. This is the reason why we chose to first validate our model on bare soil, studying specifically other processes. In particular, we studied the coupling with heat transfer, which is necessary to allow a coupling with surface energy balance and thus have the capacity of driving simulation through atmospheric forcing. That said the developed model, as it names suggest, is further developed to account for vegetation.

The opening to the results section details technical improvements in the mass balance of the FHAVeT model over the TEC which is important given this is a new model. In fact this section could also include comparison of additional performance metrics, such as improved simulation time. A comparison of the simulation time would be appropriate given the the authors link to the need for computational efficiency for data assimilation (page 8585, lines 15-19). Therefore the technical objects should also be first raised in the introduction or model evaluation sections and made an explicit component of the paper's aims.

More information on the efficiency of the Ross solution in the coupled model was added. However, for detailed work on the efficiency of the Ross solution, we refer to the works of Ross (2003) and Crevoisier et al. (2009) who demonstrate the efficiency of the Ross solution and its ability to deal with coarse grids. Moreover, since we used already published results on TEC, the computation was not done on the same hardware (neither with the same configuration of the hardware). Both computers were 'regular' desktop computers but all these points influence the results on computation times. Therefore, due to all this uncertainties, we choose not to present detailed results on computation time but orders of magnitudes.

Finally, I would like to point out that proving the efficiency of Ross solution was not the objective of the paper, but rather that using an efficient (as demonstrated in literature) model such as Ross in a coupled and more generic model would not lead to significant errors in numerical results.

On the point of the stated aims I am unsure how the new model is meant to be an improvement over the existing reference model. As far as I currently understand the paper aims to demonstrate a model with improved computational efficiency compared to the reference model without degradation of predictive skill, for use with other methodologies (e.g. data assimilation). However if this is incorrect and there is meant to be scientific / theoretical improvement then the paper needs to be revised to make its aims explicit and demonstrate the differences between the model clearly.

This is correct. We tried to be clearer about the objectives in the document.

3 Technical errors

Page 8572, line 14: "... 6 times..." should be "six"

Done

Page 8580, line 9: "...ranging..." not "rangeing"

Done

Page 8581, line 23: the choice of “...unsatisfying” as a description for the model performance seems rather inappropriate please remove.

Done

Page 8583, line 5: “As it may be observed...” seems to imply observations (i.e. data) which is not the case. Please rephrase.

Done

Page 8584, line 23: ...introducing a coupling with the atmosphere...” the model is not coupled to the atmosphere, it is forced / driven by it.

See previous comments

Reviewer 3:

General comments

The authors present a paper on the development of a new model to predict the soil moisture content during sowing (bare soil) and irrigation (soil with vegetation). The newly developed model FHAVeT is based a coupled soil–atmosphere model based on Ross fast solution for Richards' equation, heat transfer and detailed surface energy balance. The model results were tested and evaluated versus other model results (model TEC). The topic and research are within the scope of HESS. The presented paper shows difficulties in the area of a) meeting the objectives by neglecting vegetation and b) the evaluation process. The figures do not meet the criteria for scientific publishing.

Specific comments

The objective of the paper is to present a model with can predict the soil moisture content of soils having bare soil (snowing) or vegetation (irrigation – P8572, L1 & L21). While evaporation is considered (P8576, L21 & chapter 2.1), transpiration seems not to be. The model is finally evaluated at bare conditions. An evaluation with vegetation is not part of the paper.

We understand the importance of vegetation related processes (root water uptake, transpiration, crop development…) when dealing with agronomical modeling. Such consideration of the vegetation leads to strong uncertainties related to the plants "geometry" and the plant behavior in regards to water content and atmospheric forcing. Moreover, and unlike the case of soil mass balance or soil energy balance, there is no consensus on how root distribution, root water uptake and transpiration should be modelled. Therefore, we believe that the study of such processes requires a focus on their own. This is the reason why we chose to first validate our model on bare soil, studying specifically other processes. In particular, we studied the coupling with heat transfer, which is necessary to allow a coupling with surface energy balance and thus have the capacity of driving simulation through atmospheric forcing. That said the developed model, as it names suggest, is further developed to account for vegetation. Details on the choice of bare soil were added to the introduction and mention of the development of vegetation related models in present in the perspectives.

The model was evaluated at two locations for a period of less than two months. The general soil type of both locations is loam (P8580, L9 & P8591). However, this limitation in the evaluation is not mentioned in the conclusion.

As described in the paper, the two climates chosen correspond to different conditions occurring in France. In regards to the soil types, we would like to point out that the locations only refers to the climates and that four soils were simulated for each climates. The clay content ranges from 17% to 48% and the sand content from 2% to 34.3% which is quite wide for soils in agronomical applications. We however added a reminder in the conclusion that the study was limited to climate and soils occurring in France.

It is difficult to evaluate a model versus the result of a different model. The model setups were not explained, e.g. in terms of discretization, boundary conditions.

It was done in chapter 3: model intercomparison. Discretization was explained p.11 l.9-11; bottom boundary conditions were explained p.11 l.6-8; initial conditions were explained p.11 l.4-6 and top boundary conditions are linked to the coupling as shown Fig.1. Climatic forcing was detailed as well. The reason behind those choices, as explained in the paper, are justified within the paper of Chanzy et al. 2008, we therefore did not feel the need to reiterate the justification. Also, in the paper by Chanzy et al. 2008, the impact of these conditions are detailed for the model TEC and evaluated against experimental data, which is why we chose those soils, climatic forcing and setups. For clarification, the structure of the paper was modified. Subchapters were added in chapter 3 ("Model intercomparison"): "3.1 Climatic forcing", "3.2 Soil types", "3.3 Soil hydraulic characteristics", "3.4 Soil thermal characteristics", "3.5 Model setup". Subchapters were also created in chapter 4 ("Results and discussion"): "4.1 Models performance", "4.2 Water content evaluation", "4.3 Day detection capacity evaluation".

It was shown that predictive model results are not only depending on the model structure but also on the modeler's decisions during the modelling process (e.g. Holländer et al., 2014). I suggest adding a chapter where the model setup is explained in detail.

The subchapter "3.5 Model setup" contains information on boundary conditions, initial conditions and spatial discretization for both models. The choice behind those conditions are detailed and justified in the work of Chanzy et al. (2008).

We agree that modeler's choices are essential during the modeling process. However, evaluating the modeler's choice in particular in regards to boundary conditions, initial conditions or spatial discretization was not the objective of the paper. As described in the general comments, we tried to be clearer as to what the objective of our work. We however insisted on the role of the modeler in our revised version and to this end cited the work of Höllander et al. (2014). We should note however that we somehow included the role of the modeler's when evaluating the effect of neglecting vapor transport.

Moreover, evaluation of newly developed models versus results of a different model is not a strong indicator of the validity of model results. It would be of favor to test the model versus observed data.

The model TEC has itself been validated against experimental results (in Chanzy et al. 2008), which is the reason why we chose TEC as benchmark. Initial evaluation of the model included both validation against analytical results (in non-coupled conditions) and against experimental data (similarly to the work done with TEC). However, the objective of the study is to see if the degradation of the resolution method for Richards' equation using Ross solution and looser coupling allowed acceptable results. To this end, we need a benchmark considering heat and mass transfers and a way to consider different soil hydraulic characteristics. This is not possible with analytical solution and comparison with experimental data leads to questioning the parameterization (of modeler's choices) rather than the model itself. Therefore, the flexibility and the physics of TEC are appropriate to do such a comparison.

We tried to be more explicit on the choice of the benchmarking within the introduction of the TEC model.

A major part of the introduction is related to numerical fast solution (ROSS solution).
However, the manuscript misses a comparison of the computation times for FHAVeT and TEC.

More information on the efficiency of the Ross solution in the coupled model was added. However, for detailed work on the efficiency of the Ross solution, we refer to the works of Ross (2003) and Crevoisier et al. (2009) who demonstrate the efficiency of the Ross solution and its ability to deal with coarse grids. Moreover, since we used already published results on TEC, the computation was not done on the same hardware (neither with the same configuration of the hardware). Both computers were 'regular' desktop computers but all these points influence the results on computation times. Therefore, due to all this uncertainties, we choose not to present detailed results on computation time but orders of magnitudes.

Finally, I would like to point out that proving the efficiency of Ross solution was not the objective of the paper, but rather that using an efficient (as demonstrated in literature) model such as Ross in a coupled and more generic model would not lead to significant errors in numerical results.

The structure of the paper needs to be improved. The paragraph P8582, L11-21 contains a method.

The structure of the document was modified (see previous comments).

The content should not be introduced in the results chapter. Next to this point, it would be favorable to split the results and discussion chapter in two chapters. In this new chapter result, the first part might be on the model evaluation instead of the mass balance. Although the mass balance errors in TEC might be large, and *the mass balance of FHAVeT seems to be better, it is not a strong indicator since the amount of soils and locations are limited.*

The structure of the document was modified (see previous comments). See previous comments on the types of soils and locations.

The results on the model comparison are not adequately presented. If the authors use three pedotransfer functions (PTF), they might have identified differences in the results. The use of a scatter plot (P8596) does not allow studying the soil moisture timing (P8576, L6-11).

The derivations between results by the two models are only discussed on a visual basis (P8596). The use of statistical indicators can help to evaluate the data on an objective view.

We would like to point out that a scatter plot allows an exhaustive presentation of the results. We however added metrics to support the analysis of the scatter plot. In the plot, we defined a range of 0.04 m^3/m^3 around the first bisector. In the Avignon climate 1.55% of the points are out of this range for the 0-5cm layer and 0% for the 0-30 layer. In the Mons climate 6.76% of the points are out of this range for the 0-5cm layer and 1.17% for the 0-30 layer. Other data allow the evaluation of timing, namely Figure 5 and 7. It should be noted that the timing for Figure 7, is chosen as the one leading to the most significant error in all soils and PTF considered.

In regards to the study on the impact of the PTF, we would like to point out that the comparison between the results using different PTF was done with TEC in Chanzy et al. (2008). It is true that depending on the PTF chosen, the comparison between FHAVeT and TEC varies. Specifically, the two models agree very well when using Cosby PTF and show more discrepancies when using Van Genuchten – Mualem description. However, we noted that this may be due to the fact that Cosby PTF leads to high water content and little vapour transport. Therefore, we choose to focus our evaluation on a range of soil hydraulic functions, soil conditions and climatic forcing rather than on the effect of PTF. The aim of the paper is to upgrade Ross model to make its use more general and suitable in various applications.

The different PTF were identified in Figure 3.

The figures do not have an adequate quality for publication. Units are either completely missing (e.g. P8595, Figure 3), wrong (P8594, Hourly precipitation – unit: mm/hour), or invisible (1:1 line in scatter plot, P8596).
Superscripted letters should be used in figure 6 (P8598) & figure 8 (P8600).
Figure 9 (P8601) uses Drying 0day in the legend while the caption mentions Drying0.

Figures were modified accordingly to your comments (including accurate units and added information), legends were also modified when necessary.

Reference: Holländer, H.M., H. Bormann, T. Blume, W. Buytaert, G.B. Chirico, J.F. Exbrayat, D. Gustafsson, H. Hölzel, T. Krauße, P. Kraft, S. Stoll, G. Blöschl, and H. Flühler. 2014. Impact of modellers' decisions on hydrological a priori predictions. Hydrol. Earth Syst. Sci. 18 no. 6: 2065-2085.

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Development and evaluation in bare soil conditions of an efficient soil-atmosphere model (FHAVeT) based on the Ross fast solution of the Richards equation

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Abstract. In agricultural management, a good timing in operations, such as irrigation or sowing, is essential to enhance both economical and environmental performance. To improve such timing, predictive softwares are of particular interest. An optimal decision making software would require process modules which provides robust, efficient and accurate predictions while being based on a minimal amount of parameters easily available. The objective of this study is to assess the accuracy of a physically-based model with high efficiency. To this aim, this paper develops a coupled model with climatic forcing based on Ross fast solution for Richards' equation, heat transfer and detailed surface energy balance. The present study is limited to bare soil, but the impact of vegetation can be easily included. The developed model, FHAVeT (Fast Hydro Atmosphere Vegetation Temperature), is evaluated against the coupled model based of the Philip and De Vries (1957) description, TEC. The two models were compared for different climatic and soil conditions. Moreover, the model allows the use of various pedotransfer functions. The FHAVeT model showed better performance in regards to mass balance , mostly below 0.002 m and generally improved computation time. In order to allow a more precise comparison, six time windows were selected. The study demonstrated that the FHAVeT behaviour is quite similar to the TEC behaviour except under some dry conditions. The ability of the models to detect the occurrence of soil intermediate water content thresholds with a one day tolerance was also evaluated. Both models agreed in more than 90% of the cases.

1 Introduction

In agriculture a good timing of management operation such as tillage, sowing, irrigation or yielding is an important issue for both economical and environment points of view. Inappropriate irrigation scheduling may lead to water and/or crop losses, whereas using heavy engines on wet soil condition may compact soils that reduces oxygen and water flows. The decision making is multifactorial, involving work organization, meteorological forecast or soil water content. Even if progresses have been made in soil water content probe development (Evett and Parkin, 2005), their implementation remains difficult in operational context as for capturing the spatial soil variability (Evett et al., 2009) or handling in situ probes together with management operation. Modelling the soil water content dynamic is therefore an alternative to support decisions and fast computing is an important issue to obtain real time information and address the spatial variability through 3D or distributed 1D model.

As explained in the review on decision making by Ascough et al. (2008), an optimal decision making software would require process modules which provide robust, efficient and accurate predictions while being based on a minimal amount of easily available parameters. Moreover, a decision making software should allow the representation of the major processes occurring in the studied object. In regards to decision based on soil water content for agricultural management, some important processes are the water transfers in the soil/plant system and the energy fluxes in the soil and at the surface, these latter being important to determine top boundaries conditions from standard climatic data. To represent such processes, soil hydraulic properties characterisation is a critical point since they are rarely measured at the location of interest and have a strong impact on the simulations. The alternative is then to use pedotransfer functions that link those characteristics to commonly measured quantities such as the soil textural fractions.

For agricultural management purposes, capacity-based models are generally used (Bergez et al., 2001; Chopart et al., 2007; Lozano and Mateos, 2008). Such conceptual models represent soil through its water storage capacity and vertical fluxes that are governed by an overflow of a compartment towards the one just below. In general, additional processes are required to better represent infiltration and upwards flux involving empirical parameters that are site specific and need to be calibrated since they are not measurable and thus difficult to address through pedotransfer functions. Moreover, in her work, Blyth (2002) compared a conceptual model to a physically based model. The physically based model showed better performances and more versatility than the conceptual model. It should be noted, however, that the accuracy of a physically-based model is dependent on modeller's choice, for instance in regards to parametrisation or chosen processes (Hollander et al., 2014). Therefore the development of a versatile, physically-based model is of importance to allow a non-site-specific decision tool.

In the unsaturated zone, a well-known physically based description of the mass balance, in regards to water flow, is the Richards equation. The Richards equation allows a detailed description of soil water content distribution evolution as well as water fluxes inside the soil domain. It is based on measurable physical parameters which may be obtained through experimentation such as the water retention and the hydraulic conductivity. Moreover, pedotransfer functions are widely developed (Cosby et al., 1984; Rawls and Brakensiek, 1989; Wosten et al., 2001; Schaap et al., 2001) and allow description of the parameters necessary for the resolution of Richards equation using soil characteristics such as the soil texture and bulk density. Chanzy et al. (2008) demonstrated that pedotransfer functions may allow a good approximation for agricultural soil water representation even though the adequacy of pedotransfer functions close to the surface is still under discussion (Jarvis et al., 2013) especially for wet conditions when preferential flow occurs.

The Richards equation is highly non-linear leading to time-consuming numerical resolution and stability issues under some conditions such as the wetting of an initially dry medium. Numerous studies focused on the improvement of the numerical schemes (Short et al., 1995; Zhang et al., 2002; Caviedes-Voullieme et al., 2013) but it should be noted that computing codes based on Richards equation are rarely used for decision making software.

Ross (2003) proposed in his paper a fast solution of the Richards equation. This method demonstrated an accurate, robust and efficient behaviour on a variety of case studies. The solution developed by Ross (2003) has been used in different situations in the latest years, proving its efficiency against models based on the classic resolution of the Richards equation. Varado et al. (2006a) tested the solution to evaluate its efficiency and demonstrated that the model shows improved robustness and accuracy compared to analytical solutions and the model SiSPAT (Braud et al., 1995). In their work, Crevoisier et al. (2009) proposed a comparison of the solution with the Hydrus software (Simunek et al., 2008) in unfavourable conditions, demonstrating an improvement in computing time efficiency and robustness.

Thanks to its efficiency and robustness a model based on Ross solution is an interesting choice to develop a decision tool based on soil water content estimation. However, it is important to drive the model with a climate forcing and to be able to have a wide range of soil hydraulic functions (retention curve and soil hydraulic conductivity) in order to take profit of the existing pedotransfer functions.

In most of the models based on Ross solution, the introduction of climatic forcing is made through an empirical approach where the top water flux is the minimum of the potential evaporation and the maximum water flux through the top layer. Introduction of climate forcing through the surface energy balance is more straightforward and physically sound. This requires, however, to represent soil heat transfer, which may be done with a soil energy balance. Tightly coupled equations developed by Philip and De Vries (1957) may be used. In such a tightly coupled model water flow in liquid and vapour phases is strongly related to heat transfers. Haverd and Cuntz (2010) actually coupled the Ross solution with an energy and vapour transport equation based on those coupled equations. Such a development increases the number of parameters,such as those related to soil vapour diffusion, and a more complex problem resolution is required. Another possibility is to consider a loosely coupled model. In such a model, the different balances (surface energy, heat transport and water transfers) are evaluated sequentially and vapour transport is neglected. To keep a limited amount of input parameters, we prefer to develop a model based on the original Ross approach, which was widely tested in a large range of soil and water flow conditions.

The aim of this paper is to present and evaluate the evolution made on the model developed in Crevoisier et al. (2009) with the introduction of new soil hydraulic function formalisms as well as new processes (soil heat transfer and surface energy balance). At longer term, the interest would be to enlarge the scope of the soil water and heat transfer model to other processes such as root uptake, solute transport, biogeochemical reaction or soil properties dynamic. It was found that the main challenge in implementing physically based model to estimate soil water content is the evaluation of soil hydraulic properties, requiring the development of estimation strategies such as using PTF or assimilation techniques (Witono and Bruckler, 1989; Zhu and Mohanty, 2004; Medina et al., 2014). Our work focused on the innovation made in the FHAVeT model and will not consider those strategies that are already addressed in other studies (Chanzy et al., 2008). To evaluate the FHAVeT model, we therefore used a data set simulated by the TEC model (Chanzy et al., 2008) as our reference. It is based on the DeVries approach, which is physically sound to represent water transfers in the soil and at the soil/atmosphere interface. Moreover, Chanzy et al. (2008) have shown the potential of such a model for operational applications by developing an implementation strategy with limited soil characterisation. The question is then to evaluate to which extent the gain in computing efficiency and robustness brought by the Ross method, together with the physical simplification on heat and water coupling, affect the results in comparison to the TEC model that presents a stronger physical background.

In this paper, the work is limited to bare soils in order to focus on the impacts of the innovations brought in FHaveT, which are limited to the soil compartment including the interface with the atmosphere. Moreover, the very dry conditions encountered near the surface on bare soil, are the worst situations to test the lack of soil water vapour assumption. Bare soil is also an important phase in the crop cycle during which important decisions have to be taken by farmers such as crop installation (soil tillage, sowing).

2 Model description

The model FHAVeT (Fast Hydro Atmosphere Vegetation Temperature) consists in the coupling of a surface energy balance, a soil energy balance and a soil mass balance module. Models development and simulations were performed using the INRA Virtual Soil[1] platform. This platform provides an easy way to use and couple numerical modules representing processes occurring in soils. A scheme of the model is presented Figure 1. The model consists of three main modules computed sequentially in the following order: Surface Energy Balance - Soil Water Transfer - Soil Heat Transfer. As shown in Figure 1 the surface energy balance is driven by climatic forcing, soil surface temperature and soil surface water potential and it computes evaporation / rainfall and soil surface heat flux. The soil water transfer module is driven by evaporation / rainfall and computes soil water potential, water flux and water content. Finally, the soil heat transfer module depends on water flux, water content and surface heat flux and computes soil temperature.

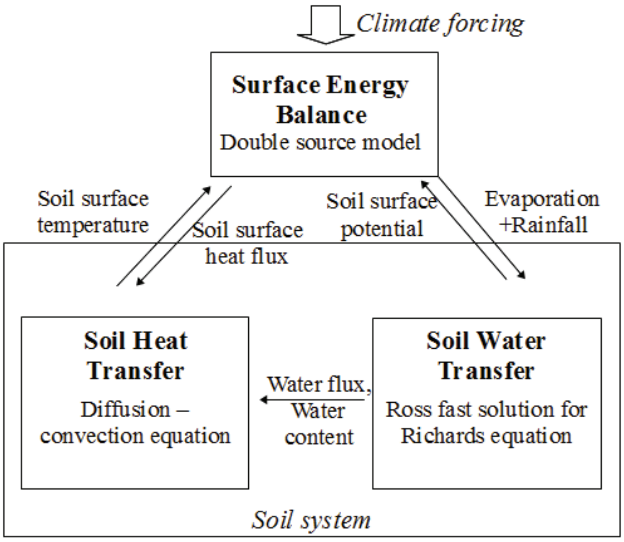

Fig. 1: The FHAVeT model coupling scheme.

2.1 Surface energy balance

An equation of energy budget (Eq. 1) at the soil surface is used to obtain the soil surface heat flux G ($\mathrm{W\,m^{-2}}$) and the soil evaporation flux E_g ($\mathrm{kg\,m^{-2}\,s^{-1}}$).

$$Rn_\mathrm{g} = H_\mathrm{g} + L_\mathrm{v}(T_\mathrm{s})E_\mathrm{g} + G \tag{1}$$

$$= -\rho_\mathrm{a} c_\mathrm{p} \frac{(T_\mathrm{a} - T_\mathrm{s})}{R_\mathrm{aH}} - L_\mathrm{v}(T_\mathrm{s}) \frac{\rho_\mathrm{a}(h_\mathrm{a} - h_\mathrm{s})}{R_\mathrm{av}} + G \tag{2}$$

In this equation, Rn_g ($\mathrm{W\,m^{-2}}$) is the net radiation, L_v ($\mathrm{J\,kg^{-1}}$) is the latent heat of vaporization and H_g ($\mathrm{W\,m^{-2}}$) is the sensible heat flux. The aerodynamic resistances for heat and vapour R_aH and R_av are calculated using the formulation by Taconet et al. (1986). T corresponds to the temperature and h to the specific humidity (mass of water in air over mass of humid air), subscripts 'a' relates to the air and 's' to the soil surface level. Moreover, ρ_a ($\mathrm{kg\,m^{-3}}$) is the air density and c_p the specific heat at constant pressure. Solving equation (1) requires climatic observation parameters, as well as the soil surface temperature and soil surface water potential calculated from the soil heat and water transfers at the previous time step and input parameters as described table 2.

2.2 Soil mass balance

Ross' fast solution for Richards equation is described in Ross (2003) and Fast Hydro, the upgraded implementation of Ross method used in this study is described in Crevoisier et al. (2009). It solves the Richards equation (3) by a non-iterative approach.

$$\frac{\partial \theta}{\partial t} = \nabla \cdot \left(K \nabla \left(\tilde{h} - z \right) \right) \tag{3}$$

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[1] All informations about the platform and how to use it and contribute can be found in the dedicated web site : http://www.inra.fr/sol_virtuel

Where θ ($\mathrm{m^3\,m^{-3}}$) is the soil water content, $\widetilde{h}$ (m) is the soil potential, K ($\mathrm{m\,s^{-1}}$) is the soil hydraulic conductivity and z (m) is the elevation. Detailed description of the Ross solution may be found in Crevoisier et al. (2009). Similarly to the code developed in Crevoisier et al. (2009), a water surface layer and time step optimization are used. The Ross solution is based on a linearisation of the mixed form of Richards equation. The solution evaluates the effective saturation ($S = (\theta - \theta_r)\,/\,(\theta_s - \theta_r)$) under unsaturated conditions and Kirchhoff potential ($\phi(h) = \int_{-\infty}^{0} K\left(\widetilde{h}\right) d\widetilde{h}$ in $\mathrm{m^2\,s^{-1}}$) under saturated conditions to allow an exact calculation of the Darcian fluxes (Crevoisier et al., 2009). However, the integration of the hydraulic conductivity is not always straightforward. Ross (2003) used exclusively Brooks and Corey formulation which is integrable analytically. Crevoisier et al. (2009) developed a numerical integration method for the use of Van Genuchten - Mualem hydraulic characteristics with $\eta = 0.5$. However, some PTF, including commonly used PTF, require the use of other formulation. For instance, the PTF of Wosten et al. (2001) or Schaap et al. (2001) implies the use of Van Genuchten - Mualem with η potentially different from 0.5. To this end, a method using beta functions was developed for integration of hydraulic conductivity as described by Van Genuchten - Mualem. This method, however, is convergent only for $\eta > -1$. Therefore, a numerical iterative method was developed for the utilisability of Van Genuchten - Mualem description with $\eta \leq -1$. A summary of the hydraulic properties that may be used in FHAVeT is done Table 1.

2.3 Soil energy balance

The soil energy balance is modelled using a simple convection diffusion model (4-5) with convection being limited to the liquid phase.

$$(\rho C)_{eq} \frac{\partial T}{\partial t} + \rho_w C_w \mathbf{q}^{\sigma} \cdot \nabla T = \nabla \cdot (\lambda \nabla T) \tag{4}$$

$$(\rho C)_{eq} = \rho_h C_{eq} = \rho_w C_w \theta + \rho_s C_s (1 - \theta_s) \tag{5}$$

Where ρ_s (ρ_w) ($\mathrm{kg\,m^{-3}}$) is density of solid (water), ρ_h is the soil bulk density, θ_s ($\mathrm{m^3\,m^{-3}}$) is the saturated water content (assumed equal to the porosity), C_s (C_w) ($\mathrm{J\,kg^{-1}\,K^{-1}}$) is the specific heat of solid (water) and λ ($\mathrm{W\,m^{-1}\,K^{-1}}$) is the soil heat conductivity. The soil heat conductivity is assumed to have a linear dependence on soil water content following equation (6) (Van de Griend and O'Neill, 1986).

$$\lambda = (1/0.654\,(\Lambda_s + 2300\theta - 1890))\,/C_{eq} \tag{6}$$

Moreover, impact of the rain on fluid transport is considered as a working hypothesis with rain having a constant temperature of 283 K.

2.4 The reference model: TEC

The TEC model (Chanzy and Bruckler, 1993) is based on the heat and mass flow theory in unsaturated media (Philip and De Vries, 1957). The resulting nonlinear partial differential equation system is solved using a Galerkin finite element method. The model is driven by a climatic forcing in case of bare soil. The model was evaluated against various experimental conditions (Chanzy and Bruckler, 1993; Aboudare, 2000; Findeling et al., 2003; Sillon et al., 2003). The major differences between the models TEC and FHAVeT are as follows:

- TEC is based on Finite Element Method for resolution of the equations, while FHAVeT uses the Ross solution for solving mass balance, energy balance being solved through Finite Difference Method.
- The coupling of soil mass and energy balances is based on a tightly coupled Philip and De Vries (1957) approach in the TEC while the FHAVeT model uses a loosely coupling, neglecting the vapour transport.

There are however others differences between the two models. The evolution of soil heat conductivity with soil water content and the aerodynamic resistances are calculated through different means. Moreover, the numerical spatial discretisations are different with a coarser mesh with FHAVeT near the surface.

3 Model intercomparison

The knowledge of soil water content profile is critical when it comes to agricultural management. Therefore, the prediction capacity in regards to soil water content of the FHAVeT model is going to be the major focus of the intercomparison. Chanzy et al. (2008) developed an implementation strategy under operational conditions when only limited information is available to describe the soil system. Their study was based on a large database covering contrasted climate regime, a large range of soil textures and four PTF. This data set was appropriate to analyse FHAVeT results under various pedo-climatic conditions and test different soil hydraulic functions.

3.1 Climatic forcing

The cases studied were chosen so as to offer a variety of climatic and soil conditions that may occur in France and in agronomic context. Two climatic sequences are used. The first one was measured at Avignon (southern France, 43.78 °N, 4.73 °E) and represents a Mediterranean climate with occasional heavy rains and long periods of dryness (Figure 2a). Wind velocity also varies strongly. The second climatic sequence was measured at Estree-Mons (northern France, 48.99 °N, 2.99 °E). It represents an oceanic climate with frequent light rainfalls and short dryness periods (Figure 2b).

Table 1: Hydraulic properties curves available in FHAVeT and Kirchhoff potential calculation methods

Retention curve	**Hydraulic conductivity curve**	**Kirchhoff potential calculation**
Brooks and Corey $S(h) = (\alpha_{\mathrm{BC}} h)^{-\lambda}$	Corey $K = K_{\mathrm{sat}} S^{\eta}$	Analytical (Ross, 2003)
Linear $S(h) = \exp\left(\alpha_{\mathrm{G}}\left(h - h_e\right)\right)$	Linear $K = K_{\mathrm{sat}} S$	Analytical (Crevoisier et al., 2009)
Van Genuchten $S(h) = \left(1 + \lvert\alpha_{\mathrm{VG}} h\rvert^{n}\right)^{-m}$	Mualem $K = K_{\mathrm{sat}} S^{\eta} \left[1 - \left(1 - S^{1/m}\right)^{m}\right]^2$	Numerical ($\eta = 0.5$) (Crevoisier et al., 2009) † Beta functions ($\eta > -1$) †† Numerical ($\eta \leq -1$) ††
modified Van Genuchten $S(h) = \frac{1}{S_{\mathrm{M}}}\left(1 + \lvert\alpha_{\mathrm{VG}} h\rvert^{n}\right)^{-m}$ $S_{\mathrm{M}} = \left(1 + \lvert\alpha_{\mathrm{VG}} h_e\rvert^{n}\right)^{-m}$	Mualem $k_r = \frac{S_{\mathrm{M}} S^{\eta}}{k_{\mathrm{M}}} \left[1 - \left(1 - (S_{\mathrm{M}} S)^{1/m}\right)^{m}\right]^2$ $k_{\mathrm{M}} = {S_{\mathrm{M}}}^{\eta} \left[1 - \left(1 - S_{\mathrm{M}}^{1/m}\right)^{m}\right]^2$	Numerical ($\eta = 0.5$) (Crevoisier et al., 2009) † Beta functions ($\eta > -1$) †† Numerical ($\eta \leq -1$) ††
Van Genuchten $S(h) = \left(1 + \lvert\alpha_{\mathrm{VG}} h\rvert^{n}\right)^{-m}$	Corey $K = K_{\mathrm{sat}} S^{\eta}$	Beta functions ††

† Integration method upgraded
†† New feature in the FHAVeT model

In order to study specific features of the two climatic sequences, six time windows (TW) were selected (Figure 5). TW 1 and 2 are chosen within the first drying period of the Avignon sequence with TW 1 showing strong wind conditions and TW 2 little wind conditions. Indeed, Chanzy and Bruckler (1993) demonstrated that wind has an influence on vapour transport with lower vapour flow when the convective part of the climatic demand is stronger. TW 3 is selected during the heavy rain period of the Avignon sequence. TW 5 covers the drying conditions of the Estree-Mons climate. Finally, TW 4 and 6 were chosen during wet periods of the Estree-Mons sequence, respectively before and after the dry period. A summary of the averaged climatic conditions during those 6 time periods is shown Table 3.

3.2 Soil types

Four soils from the sites of Estree-Mons and Avignon with various textures, ranging from silty loam to silt clay loam (Table 4) were chosen for the study.

3.3 Soil hydraulic characteristics

To validate the versatility of the model, the three integration methods (Table 1) were solicited through the use of three different PTF. The pedotransfer function developed by Cosby et al. (1984) offers parameters corresponding to a Brooks and Corey set of hydraulic properties and therefore requires the use of analytical integration in the software. The pedotransfer function developed in Rawls and Brakensiek (1989) allows deriving of Van Genuchten - Mualem hydraulic properties parameters with the hypothesis of shape parameter η equals 0.5. Therefore integration with beta functions may be used. Finally, the pedotransfer function of Wosten et al. (2001) also derives Van Genuchten - Mualem parameters, but shape parameter η obtained are usually below -1, therefore numerical integration is necessary. All three functions require the same parameters, which are the textural characteristics of soils, summarised in Table 4.

3.4 Soil thermal characteristics

Thermal characteristics of the different soils were considered dependent on volumetric soil water content. The heat capacity is calculated as the mean of soil and water capacities weighed by relative volumes. In the FHAVeT model, the heat conductivity dependence on the soil water content is obtained through equation (6). The thermal inertia at saturation Λ_s ($\mathrm{J\,m^{-2}\,K^{-1}\,s^{-1/2}}$) has been tabulated against soil textures by Van de Griend and O'Neill (1986). In the TEC model, the evolution of heat conductivity is obtained through the De Vries (1963) description.

Table 2: Input climate forcing and parameters for the FHAVeT model

Climatic forcing data		
Short-wave incoming radiation RG	$W.m^{-2}$	
Long-wave incoming radiation RA	$W.m^{-2}$	
Atmospheric temperature at reference height T_a	K	
Atmospheric pressure p_{atm}	Pa	
Air vapour content e_a	Pa	
Wind velocity at reference height U_a	$m.s^{-1}$	
General properties		
Water density ρ_w	$1000\ kg.m^{-3}$	
Air density ρ_a	$kg.m^{-3}$	Function of temperature and pressure
Latent heat of vaporization L_v	$J.kg^{-1}$	Function of temperature
Specific heat of dry air at constant pressure c_p	$1004\ J.K^{-1}.kg^{-1}$	
Specific heat of water C_w	$4181\ J.kg^{-1}.K^{-1}$	
Surface energy properties		
Ground surface albedo α_g	0.20-0.30	Function of surface water content
Ground surface emissivity ε_g	0.96	
Roughness length for momentum z_{om}	0.002 m	
Roughness length for heat z_{oh}	m	Calculated with Brutsaert (1982) formula
Soil hydraulic properties		
Saturated volumetric water content θ_s	$m^3.m^{-3}$	
Residual volumetric water content θ_r	$m^3.m^{-3}$	
Water retention curve parameters		
Hydraulic conductivity curve parameters		
Soil thermal properties		
Soil heat conductivity λ	$W.m^{-1}.K^{-1}$	Function of soil water content
Soil heat capacity C_λ	$J.kg^{-1}.K^{-1}$	Function of soil bulk density

Table 3: Climatic forcing summary for the selected time windows (TW)

Case	Site	Start date	End date	Duration	Temperature	Precipitation	Mean wind velocity
TW1	Avignon	23/09/97	30/09/97	168 h	14.9 °C	0 mm	$5.14\ m.s^{-1}$
TW2	Avignon	30/09/97	05/10/97	120 h	15.3 °C	0 mm	$0.65\ m.s^{-1}$
TW3	Avignon	11/10/97	12/10/97	24 h	15.9 °C	55 mm	$1.25\ m.s^{-1}$
TW4	Mons	04/10/04	08/10/04	91 h	15.9 °C	16 mm	$4.08\ m.s^{-1}$
TW5	Mons	16/10/04	25/10/04	214 h	14.9 °C	1 mm	$3.09\ m.s^{-1}$
TW6	Mons	26/10/04	31/10/04	120 h	12.9 °C	11 mm	$3.06\ m.s^{-1}$

3.5 Model setup

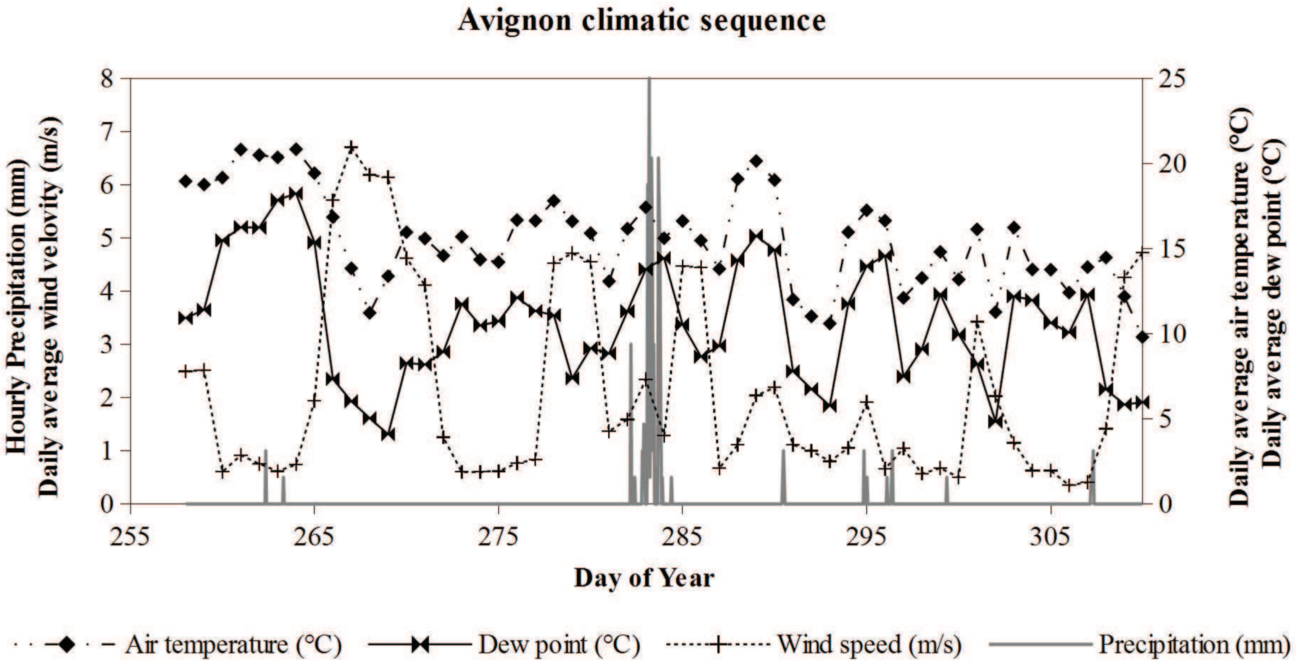
Initial values for soil matric potential and soil temperature used in the FHAVeT model were the ones derived using TEC model from a preliminary climatic sequence (Chanzy et al., 2008). Constant matric potential (-3.33 m) and temperature (293 K) are considered at the bottom of the studied domain for both models as used in Chanzy et al. (2008).

The one-dimensional mesh used in FHAVeT is homogeneous with a cell thickness of 2 cm and a total soil thickness of 80 cm while the mesh used in TEC is refined close to the surface with element thicknesses ranging from 0.6 cm to 5 cm. The number of cells is identical for both models.

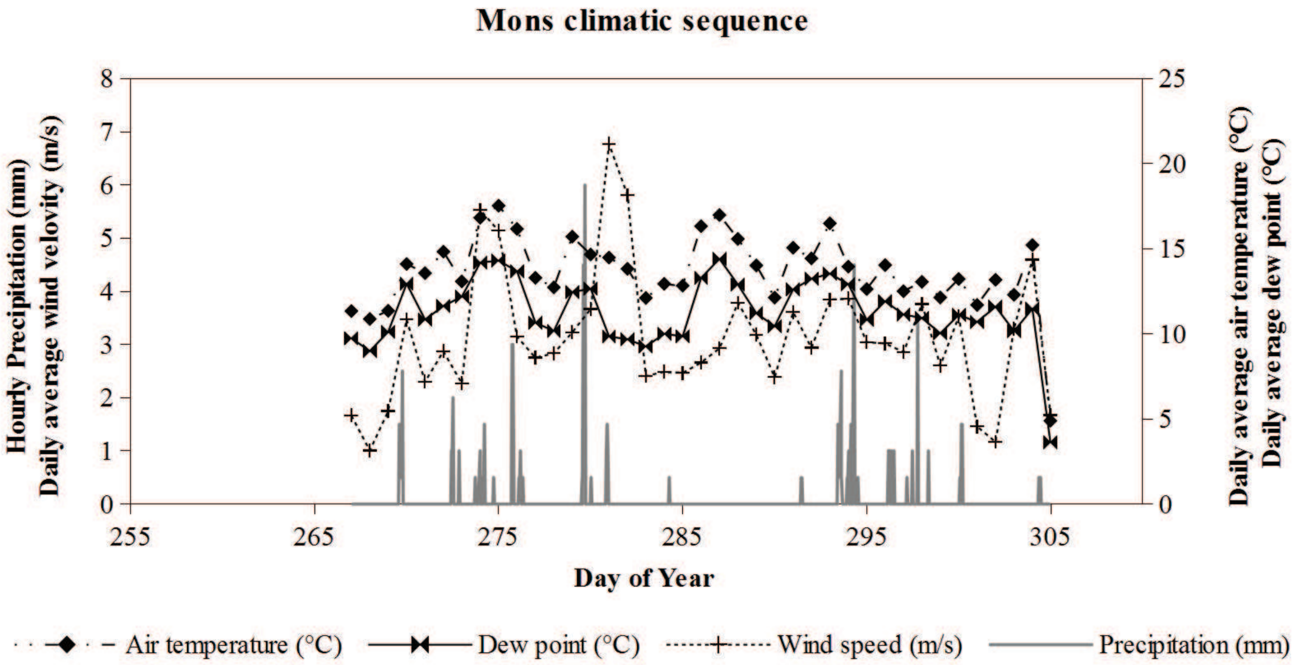
4 Results and discussion

4.1 Models performances

A study on the efficiency of the Ross solution against classic resolution of Richard’s equation under various boundary con-



(a) Avignon sequence



(b) Estree-Mons sequence

Fig. 2: Climate forcing - Precipitation, air temperature, dew point and wind velocity at 2 m height

Table 4: Soil characteristics for comparative study, from Chanzy et al. (2008)

Soil ID	Depth (m)	Texture	Clay %	Sand %	Bulk density (kg.m^{-3})	Organic Matter %
AL-SiL	0.00-0.10	Silt loam	17.00	34.30	1240	1.50
	0.10-0.40		17.00	29.20	1280	1.50
	0.40-0.80		17.00	29.20	1460	1.00
AL-SiCL	0.00-0.10	Silt clay loam	38.90	5.30	1300	2.50
	0.10-0.40		39.70	4.60	1350	2.50
	0.40-0.80		48.10	2.00	1600	1.00
MO-SiL	0.00-0.33	Silt loam	14.50	5.20	1280	2.10
	0.33-0.80		25.20	3.00	1520	0.90
PO-SiCL	0.00-0.10	Silt clay loam	27.20	11.00	1290	2.40
	0.00-0.25		27.20	11.00	1400	2.40
	0.25-0.80		27.20	11.00	1600	1.00

ditions was done in Crevoisier et al. (2009). In their work, they demonstrated that Ross solution allowed a computation time five time per grid cell lower (in average) compared to a regular solution of Richards equation. Similar outcomes, (computation time of around a couple minutes in FHAVeT case and a few tens of minutes if TEC case) were observed in this study. It should be noted noted that in one case (AL-SiCL with the Wosten pedotransfer functions and under the Avignon climate), the computation time using FHAVeT remained in the same order of magnitude than the one of TEC.

To compare the numerical accuracy of both models, a calculation of mass balance was performed . The mass balance absolute error was computed as the absolute difference between cumulated in and outflow of the soil domain and the soil water storage evolution from initial state at each time step. The maximal value along time for the mass balance error is represented Fig. 3. As shown in Fig. 3 the TEC mass balances are not always respected (error lower than 0.01 $\mathrm{m}^3\,\mathrm{m}^{-2}$) due to strong water potential near the surface in dry conditions. FHAVeT offers improved results in regards to mass balance compared to the TEC model. In most cases the absolute mass balance error was below 0.002 $\mathrm{m}^3\,\mathrm{m}^2$ with only one case being higher. In this particular point, corresponding to the soil AL-SiCL with the Wosten pedotransfer functions and under the Avignon climate, both the computing time and the mass balance (0.008 $\mathrm{m}^3\,\mathrm{m}^{-2}$) error were too large. As explained in the model description, the variables calculated are different when a cell is saturated (Kirchhoff potential) or unsaturated (effective saturation). Therefore, when a cell is going from unsaturated to saturated state (or reversely), the calculation undergoes an error. For the hydraulic conductivity curves from Wosten et al. (2001), there is a very steep non linear variation of permeability close to the saturation. This leads to a slow numerical calculation of the permeability close to saturation state as well as a strong discrepancy between the soil saturated and slightly unsaturated state flow characteristics. All these considerations leads to a heightened probability of an "oscillation" to occur between saturated and unsaturated states and the consequent error accumulation. An improvement of the numerical integration method should, however, improve the computation time and allow the use of a more constraining numerical tolerance.

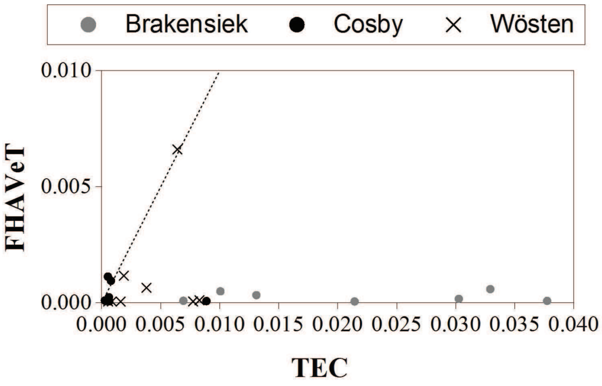
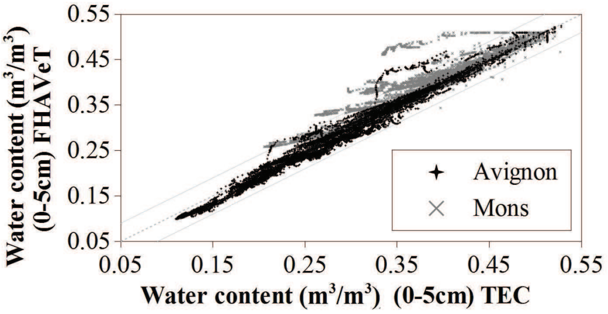


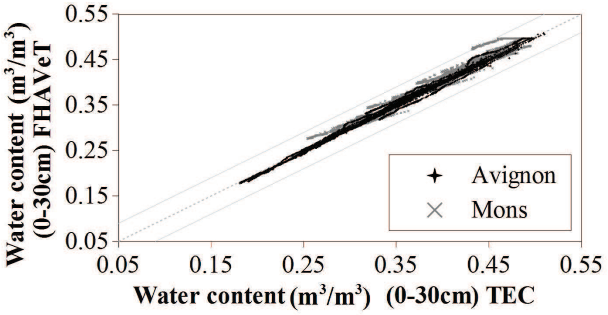
Fig. 3: Maximum absolute error in mass balance (in water cubic meter per unit soil surface) - Comparison between models. The dotted line corresponds to the 1:1 line.

4.2 Water content evaluation

Figure 4 shows the comparison of all cases studied between soil water content of both models for the 0-5 cm and 0-30 cm soil layers. A tolerance of 0.04 $\mathrm{m}^3\,\mathrm{m}^{-3}$ is shown. The models show generally good agreement. For the 0-5 cm layer, only 1.55% (6.76%) of the results are out of the tolerance zone for the Avignon (Mons) climate. The results go down to 0% (1.17%) for the 0-30 cm under the Avignon (Mons) climate.



(a) 0-5 cm layer



(b) 0-30 cm layer

Fig. 4: Comparison of soil water content between models FHAVeT and TEC for all models every 2 hours

To study the conditions of the divergences between the two models, the evolution of soil water content with time for surface layer and in one particular simulation is shown Figure 5. This figure shows that the most significant discrepancy between the two models seems to occur during TW 5, that is during the drying period of the Mons climate.

In order to extend this analysis to all cases studied, Figure 6 shows the histogram of the absolute difference distribution between the water content averaged over a defined soil depth (0-5 cm and 0-30 cm) for both models over each time window. The comparison takes into account all pedotransfer functions.

It can be clearly observed that under wet conditions (TW 3, 4 and 6) the two models led to similar results with the absolute difference in averaged water content being lower than 0.01 $\mathrm{m^3\,m^{-3}}$ for around 80% of the time in the 0-5 cm soil layer and always below 0.03$\mathrm{m^3\,m^{-3}}$ in the 0-30 cm soil layer. However, under dry conditions (TW 1,2 and 5) the difference between the two models is more consequent. This is especially true in TW 5, where there is little rain for a long time (1.5 mm in 12 days), which leads to absolute water content difference going over 0.1 $\mathrm{m^3\,m^{-3}}$. Since the discrepancies between the models mostly occur during drying, the lack of vapour transport is likely to be a source of error. In order to investigate

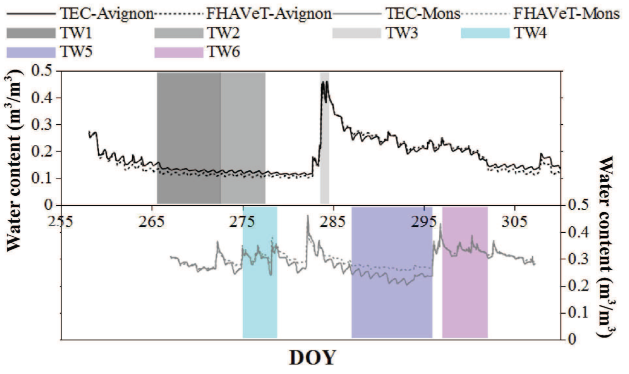
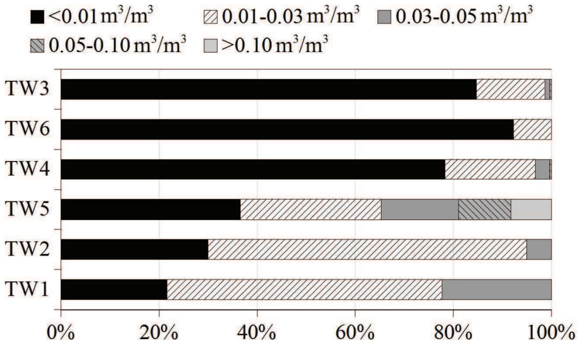
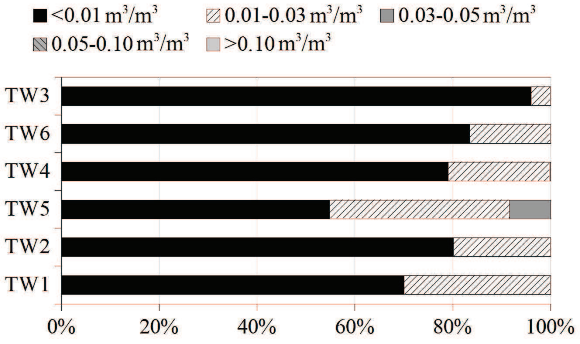


Fig. 5: Soil water content evolution in time, for the 0-5 cm layer, comparison between models - Soil AL-SiL, PTF - Wosten. Avignon climate on the top and Mons climate at the bottom.



(a) 0-5cm layer



(b) 0-30cm layer

Fig. 6: Absolute water content difference distribution between the developed model and TEC for each climatic case study

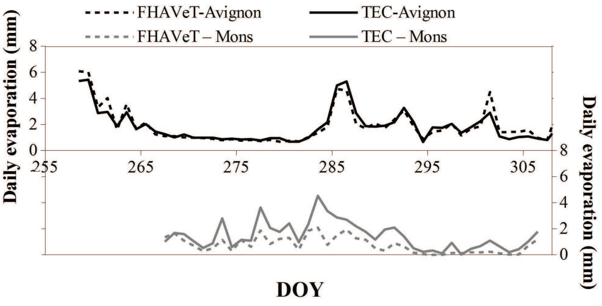


Fig. 7: Daily evaporation (in mm) evolution in time, comparison between models - Soil AL-SiL, PTF - Wosten

the role of vapour transport, the evaporated flux was plotted in Figure 7 for one case. This case shows representative behaviour of all soils and climates studied where there is discrepancy between the two models (with the exception of the case showing numerical issues).

As shown in Figure 7, the model FHAVeT tends to underestimate the evaporation of soil under Mons climate drying conditions and consequently leads to a higher soil water content in the observed soil layer. The errors are larger in the 0-5 cm layer than in the 0-30 cm layer which tends to demonstrate that the impact of vapour transport is most important close to the surface. Such considerations are further observed in Figure 8. This figure compares three water content profiles for each model. Under dry conditions and Mons climate (during TW5) the profiles are comparable below 30 cm and their discrepancy increases when depth decreases. Moreover, the water content simulated by the TEC model during the drying phase is significantly smaller than the one computed with The FHAVeT model. Therefore the driest conditions at the soil surface must be balanced by vapour flow to produce greater evaporation rate. Under Avignon climate, both models led to similar evaporation rate even in very dry condition and therefore the water content profile (Figure 8) are comparable even close to the surface. In such dry conditions, Chanzy (1991) showed that water vapour flows are much smaller than that at the beginning of the drying phase. Therefore, intermediate water content conditions, such as the ones encountered under Mons Climate, lead to the the strongest discrepancies. After a rainy period, the profile almost seems to be recovered in TW6. While the maximal error between the two models in water content is of 0.087 $\mathrm{m^3\,m^{-3}}$ in the dry state (TW5), it is of 0.015 $\mathrm{m^3\,m^{-3}}$ eight days later. This result shows that the local error generated during the drying is diluted along the soil profile. Moreover, the error in water amount of the whole domain is reduced by 27 % (from 0.0071 $\mathrm{m^3\,m^{-2}}$ in the dry state to 0.0052 $\mathrm{m^3\,m^{-2}}$), showing a partial recovery of soil water content.

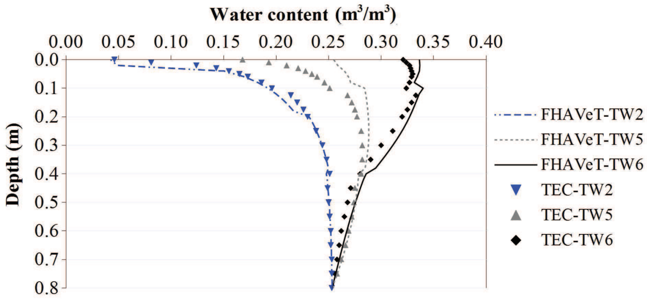


Fig. 8: Water content profiles in TW 2 (dry conditions - Avignon climate - DOY 275) TW 5 (dry conditions - Mons climate - DOY 292) and 6 (wet conditions - Mons climate - DOY 300) for soil AL-SiL, Wosten pedotransfer function

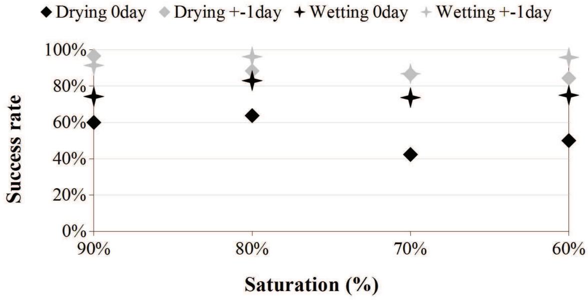


Fig. 9: Day detection success rates. Drying 0day and Wetting 0day show the amount of identical day detection for both models during drying and wetting respectively. Drying+-1day and Wetting+-1day show the success rate for day detection when there is less than 1 day difference between the two models.

4.3 Model ability for water content thresholds estimation

In decision-support software, soil water content thresholds can be applied as criteria for decision on agronomic management such as irrigation or tillage and harvesting to prevent soil compaction (Saffih-Hdadi et al., 2009). Therefore, the ability of a model to accurately detect the day when the soil water content status reaches such thresholds is essential. Figure 9 shows the amount of accurate dates (considering TEC as a reference) at which a given saturation value (for the top 30 cm layer) was detected either from dry to wet conditions (wetting) or from wet to dry conditions (drying) as well as day detection with a one day tolerance.

Due to the little amount of saturation conditions below 50% the lowest threshold showed in Figure 9 is 60%. It can be observed that thresholds are detected at the same date for two thirds of the cases at higher saturation (thresholds 90% and 80%) and a little over half of the cases for thresholds 70% and 60% during drying. The success rate is much higher during wetting. Moreover, the success in day detection with a one day tolerance is quite high in wet conditions (thresholds of 90%).

Important day detection delay (or advance) of over three days have occurred in only 0.8 % of the cases and significant day detection misses (when the threshold is reached for more than three days) in 1.4 % of the cases. The day detection inaccuracy may have different causes. The case where mass balance error is high has lead to an early detection in the FHAVeT model. This is likely due to the numerical error as the discrepancy between soil water volume between the two models and the mass balance error in the FHAVeT model are quite similar. The other cause of day detection miss or delay could be the lack of vapour transport. Indeed, all other day detection misses or delay appear during the drying period and especially TW 5. As mentioned previously, this period corresponds to intermediate water condition that led to the largest discrepancy in evaporation and thus soil moisture. Therefore, in a tightly coupled model such as TEC, the soil is allowed to dry at a higher pace leading to earlier day detection than in a loosely coupled model such as FHAVeT.

5 Conclusions

FHAVeT extends the model developed by Ross (2003) and improved by Crevoisier et al. (2009) by introducing a coupling with the atmospheric conditions and by considering a wider range of soil hydraulic functions in order to take profit of commonly used pedotransfer functions. The coupled model is based on existing process modules and uses the coupling technology offered by the soil virtual modelling platform to make the software development easier. As a consequence, a loose coupling between soil heat and mass flow is introduced leading to ignore water vapour flows. Moreover, water and heat flow are computed sequentially. The model developed was compared to a reference model, TEC, under two climates typical of France and using four soils textures from different area in France.

The model demonstrated good efficiency and improved mass balance conservation in comparison to the model TEC with the exception of one particular condition. In that case, the soil characteristic curves (soil water retention and relative permeability) are highly non-linear and lead to an “oscillatory” behaviour between saturated and unsaturated state, accumulating numerical errors.

The loose coupling lead to little error in rainy conditions. Under dry conditions with the Avignon climate the error is larger, which was to be expected due the more important role of vapour transport. However, the simulated discrepancy is limited to the firsts centimetres and therefore concerns a rather limited volume of water.

Since the developed model is aimed at being a support for decision making software, it is important that it accurately simulates threshold criteria. The FHAVeT and TEC models are in good agreement for around 90% of the day detections with a one day tolerance. Considering the modelling parameters and initial conditions uncertainties in field application, such a tolerance seems to be acceptable. Moreover, due to the lesser computing time (Crevoisier et al., 2009) required by the Ross solution, the FHAVeT model is a much better candidate than TEC for improvement techniques of parameter and initial conditions description such as data assimilation.

However, under drying conditions, the FHAVeT model may fail to correctly simulate the soil drying, especially close to the surface. In such conditions, wrong decisions may be taken even though the model allowed good recovery of the soil water content after a rainy period. It is consequently important to fully identify the specific climatic and soil history conditions that lead to inaccurate description of the soil behaviour in regards to water content. To do so, a wider evaluation of the model, as well as a comparison with experimental field values require further work. Future improvement of the model include a better numerical integration method in order to deal with highly non-linear soil characteristic functions as well as coupling with water transfers due to vegetation.

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